the partial pressure of each component taken separately." So many of my correspondents, including Lord Rayleigh, have questioned the grounds of this statement, which is the crux of the whole problem, that it may be of interest to explain my reasons more fully.

Arguing on the analogy of a gas, it is evident that the vibrations of radiation must be regarded as adiabatic, and cannot satisfy pv=constant, unless the value of the index γ (the ratio of the total energy E+pv to the intrinsic energy E) is equal to unity, which is impossible. We conclude either that the analogy is false, or that, if the vibrations are adiabatic, the ratio may have different values for different frequencies. Since the index γ is equal to $(E+pv)/E$, it is obvious that, to be consistent, we must have $p=(\gamma-1)E/v$, which agrees with Lord Rayleigh's result for monatomic gases, and may be true generally for the pressure of adiabatic vibrations.

According to my theory, radiation consists of the vibration of equal elementary units (Faraday tubes associated with ionic pairs) each possessing the same angular momentum, but having intrinsic energy proportional to the frequency ν and independent of the temperature T. The pressure is assumed to be equally divided between the molecular units according to the gas law pv=RT, because this gives the simplest possible explanation of the exponential term $e^{-b\nu T}$ in the radiation formula as a direct consequence of Carnot's principle, and because equipartition of pressure is the most universal condition of equilibrium in physics.

It follows that the ratio of the pressure to the energy density, denoted by $\gamma-1$, must be of the form $T/b\nu$, which is different for different frequencies at the same temperature, but gives the mean value 1/3 for full radiation. The possibility of having different values for this ratio is explained by the fact that the vibrations are adiabatic, and the correction thus introduced into the theory of radiation is in this respect analogous to that introduced by Laplace into the Newtonian theory of the propagation of sound.

The assumption here made admits of a fairly simple experimental test, such as the following. Divide the radiation from a source, such as an arc light, into two parts of different frequencies. Compare the total energies and pressures by suitable means. The ratio of the pressure to the energy should be the same for each part on Maxwell's theory. On my theory, the part of lower frequency should have the higher pressure in a determinate ratio. I hoped to be able to try this crucial test before publishing even an outline of my theory, but the rapid extension of the Imperial College in recent years has left me insufficient leisure for so exacting an experiment, though it might not present serious difficulty to an expert in the measurement of radiation pressure.

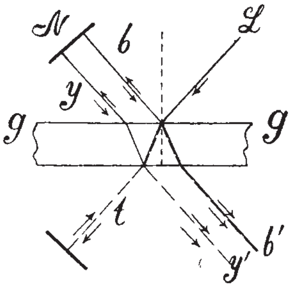
There are many other points in so brief a sketch which may require further elucidation, but these must be postponed. In the meantime I hope I have succeeded in demonstrating at least the possibility, if not the probability, of the fundamental assumption of my theory.

H. L. CALLENDAR.

Imperial College of Science, S.W., December 10.

Scattering in the Case of Regular Reflection from a Transparent Grating: an Analogy to the Reflection of X-Rays from Crystals.

1. *The Phenomenon.*—No doubt the following phenomenon has been noticed before, but I have seen no description of it. If a vertical sheet of white light L from a collimator is reflected from the two faces of a plateglass grating, having about 10,000 or more lines to the inch, g being the ruled face, the two beams b and y going to the opaque mirror N are respectively vividly blue and brownish-yellow. In other words, more blue light is regularly reflected from the ruled surface than is transmitted, and more reddish light transmitted than is reflected. Since the plate grating is not quite plane parallel, two of the four rays, b^1 and y^1, are seen in the same colours in the telescope. This is a great convenience in adjusting the displacement interferometer, where the spectra from b alone are wanted, and the y ray may be screened off at N, while the other y^1 has no spectrum.



The transmitted rays, t, after reflection show very little difference, the one reflected at g being perhaps slightly yellowish as compared with the other.

The spectra from b and y, if compared one above the other, are practically identical. The difference is not sufficiently marked to be discerned by the eye. Multiple reflection from the two faces gave no further results.

Finally, to be coloured blue, the beam must be reflected from the air side and not from the glass side, where but little appreciable effect is produced. If the grating is turned 180°, both the b and y rays are nearly white, while the t rays now correspond to the b and y rays, and are vividly coloured.

Outside the ruled surface and with any ordinary unruled plate of glass, all images are, of course, white. I mention this merely since one might suppose the absorption or colour of the glass to have something to do with the experiment. The film grating, where sharp reflection takes place from the glass and not appreciably from the film, does not show the phenomenon.

2. *Explanation.*—Scattering is usually and perhaps essentially associated with diffuse reflection. The present phenomenon, however, is strictly regular reflection—*i.e.* there is a wave front, for the blue and yellow slit images are absolutely sharp in the telescope. This is the interesting feature of the phenomenon, which associates it at once with the recent famous discovery of Friedrich, Knipping, and Laue relative to the reflection of X-rays from the molecules of crystals, and it is for this reason that I direct attention to it.

In case of the grating the sources of scattered light waves are not only identical as to phase, but these sources are at the same time equidistant. Hence collectively they must determine a wave front of somewhat inferior intensity, but otherwise identical with the wave front of normally reflected or diffracted light—*i.e.* the wave fronts of regularly reflected and scattered light are superposed.

Moreover, if the grating is turned in azimuth even as much as 45° on either side of the impinging beam (after which the many reflections and diffractions seriously overlap), the blue and brown colorations are distinctly intensified. This also is in accordance with anticipations; for the number of lines which are comprehended within the lateral extent s of the narrow beam L, as the angle of incidence i is varied, increases as $s \sec i$; whereas the lateral extent of the reflected beam is no larger than that of the impinging beam. Hence there should be increased intensity of scattered light in the ratio of $\sec i$, or increasing markedly with i from 1 for $i=0°$, to ∞ for $i=90°$. In other words, the scattering lines of the grating are virtually more densely disseminated when i increases.

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For the case of light reflected from the inside of the glass plate the evidence to be obtained from colour is too vague to admit of definite statements. I have not therefore attempted it.

Brown University, Providence, U.S.A.
CARL BARUS.

Fractured Flints from Selsey.

I AM astonished to read in the abstract of the Proceedings of the Geological Society of London, No. 947, giving an account of the meeting held on November 19, 1913, the following statement:—

"Prof. Sollas exhibited a series of specimens to illustrate the production of 'rostro-carinate' forms of flint by natural agencies. . . . The great majority were obtained by Mr. E. Heron-Allen from the beach of Selsey Bill, and it was to these that attention was especially directed. If they were all of human workmanship—Sir E. Ray Lankester's contention—there would be no difficulty in accounting for the characters which they possess in common."

I do not know whether Prof. Sollas is responsible for these words or not. But, in any case, I must state in the most unqualified way that they contain an assertion which is absolutely contrary to fact. I have never published any "contention" about flints from Selsey Bill, excepting a brief description in my paper in the Phil. Trans., Series B, vol. 202 (read on November 16, 1911), of one large rostro-carinate implement and one large pyramidal hammer-stone from that locality. To this brief description follows the remark: "Other specimens of a less decisive character have been found."

The assertion that it is my contention that any of the flints (much less "all") obtained by Mr. Heron-Allen, which I have examined, excepting the two briefly described by me, are of human workmanship is the creation of Prof. Sollas's imagination. I should be glad if Prof. Sollas would state where and when I have been guilty of the contention which, according to the Geological Society's report of his communication, he does not hesitate to attribute to me. I, of course, do not suppose that Prof. Sollas attributes a rash "contention" to me in order that he may have the satisfaction of showing it to be rash, and such as to render what I really have said unlikely to be well founded. At the same time, I think I am entitled to call upon Prof. Sollas either to cite "chapter and verse" in which I have made the specific contention which he supposes I have made, or to express some regret for a misrepresentation which I can only account for by a regrettable lapse of attention on his part in the conduct of an important scientific discussion.
E. RAY LANKESTER.

December 3.

I HASTEN to express my extreme regret at having attributed to Sir E. Ray Lankester an opinion which he does not hold.

In the quotation he gives from his paper in the Philosophical Transactions, Sir E. Ray Lankester omits the concluding sentence, "I hope to publish figures of the Selsey Bill specimens at no distant date." I understood this (naturally it seems to me) to apply to all the specimens, and thus concluded that the difference between the more and the "less decisive" was not so important as, upon the omission of the concluding sentence, it appears to be.

When I selected from Mr. Heron-Allen's collection some of his best specimens, by no means all, he assured me that they had been examined by Sir E. Ray Lankester, and pronounced by him to be of human workmanship, a judgment which appeared to me so natural and consistent with Sir E. Ray Lankester's point of view that no suspicion of a misunderstanding crossed my mind. Had I been in doubt I should have taken the precaution to ascertain from Sir E. Ray Lankester his opinion beforehand.

I am glad that Sir E. Ray Lankester acquits me of any intentional unfairness. I thought, and still think, that of the alternatives I proposed, the one I unfortunately attributed to him was the more logically defensible, but in this again I may be mistaken.

I have written to the secretary of the Geological Society requesting him to correct my statement and to add an expression of my regret to be published in the Quarterly Journal of the society.

December 7. W. J. SOLLAS.

The Structure of the Atom.

I CONCUR with Prof. Rutherford (NATURE, December 11, p. 423) that the work by Moseley in the current number of the *Philosophical Magazine*, which was not published, and was quite unknown to me when I wrote my letter (NATURE, December 4, p. 399), is an important independent confirmation by new physical methods of van der Broek's suggestion. As, however, in a paper published eight months previously (*Jahr. Radioaktivität und Elektronik.*, 1913, x., 193), I had represented in a diagram the places in the periodic table from uranium to thallium, with the mass as the ordinate and the charge as the abscissa, showing that there is unit difference of charge between successive places, I wish to take exception to Prof. Rutherford's statement "that the strongest and most convincing evidence" in support of van der Broek's hypothesis will be found in Moseley's paper. The view had already been far more simply and convincingly established from the chemical examination of the properties of the radio-elements, notably by A. Fleck in this laboratory. Moseley's conclusions are a welcome confirmation, by an independent method, for another part of the periodic table. It can only be described as the strongest and most convincing evidence if the prior chemical evidence is altogether ignored.
FREDERICK SODDY.

Physical Chemistry Laboratory, University of
Glasgow, December 12.

The Occurrence of Pilchards in the Eastern Half of the English Channel.

IT is now generally recognised by those who have been interested in the question, that the inshore migration of pilchards in the western fishery area during the summer and autumn of the present year, has presented certain features, which may possibly be attributed to somewhat unusual conditions of food supply and other determining factors. It is therefore a matter of some importance to note that according to the statement of local fishermen, occasional catches of some thousands of pilchards have been made in drift nets off Brighton, Ramsgate, Deal, &c., for several months past.

In the early part of September we examined at Brighton some specimens taken from a catch of about four thousand, and now within the past fortnight, by the courtesy of Mr. E. W. Cowley, the superintendent of the Brighton Marine Aquarium, we have been enabled to ascertain that the fish were still present in the same area. For according to the statement of this gentleman a catch of three thousand was made by a local drifter about two miles off Brighton on November 27, three specimens of which we examined and found to be males with generative organs in "half-ripe" condition.
HAROLD SWITHINBANK.
G. E. BULLEN.

London, December 10.

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